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**Uncontrived Messiah or Passover Plot? A Study of a Johannine
Apologetic Motif**

James F. McGrath

In 1965 Hugh Schonfield wrote a book entitled *The Passover Plot*¹, in which he argued that Jesus did in fact ostensibly fulfil a number of important Old Testament prophecies. However, Schonfield suggested that this was the case not because of some divine plan being acted out in history, but precisely because Jesus believed himself to be the Messiah, and thus orchestrated events, with the help of certain disciples whom he could trust and a few influential friends, in such a way as to enable him to carry out actions and participate in events which would demonstrate his messiahship. The aim of this article is not to weigh the merits of Schonfield's thesis in relation to the historical Jesus, but to suggest that the author of the Fourth Gospel was concerned to argue against similar claims being made by some of his Jewish contemporaries. John's arguments against such a view unfortunately will not (at least on their own) answer for us today the historical question of what actually happened, but a study of this theme can still be valuable in illuminating an important and frequently overlooked feature of the Johannine Gospel.

There can be little doubt that the Fourth Gospel reflects to a large extent the apologetic and polemical interests of the Johannine community². Throughout the Gospel one finds a conflict between Jesus and the Jews, primarily over christology, in the course of which the figure of Jesus is made to address the issues confronting

1 First published by Hutchinson, 1965; reprinted London: Futura Publications, 1977.

2 The term 'Johannine community' as used in this paper does not imply the acceptance of a particular reconstruction of that community's history, but simply refers to the church of which the evangelist was a part and whose experiences are reflected in the Gospel.

the contemporary church³. The focus of most recent studies has been John's 'high' christology, which is obviously one of the most fascinating and intriguing aspects of the Gospel. Yet it is important to recall that this christology is the end result of a long process of development, spurred on for the most part by the conflict in which the community was involved and their need to engage in apologetic. Within the Fourth Gospel we find evidence not only of this later stage in the development of the community and its beliefs, but also of earlier stages⁴. At first, the community's conflict with the Jewish leaders of the synagogue was not about the attribution of a high christology to Jesus, and the question of whether he was rightly attributed a status equal to God, but rather was about the attribution of *any* christology to the figure of Jesus of Nazareth, i.e., the claim that Jesus was in fact the Jewish Messiah⁵. Our study will

3 Numerous studies have been made of this aspect of the Fourth Gospel. See especially J.L.Martyn, *History and Theology in the Fourth Gospel*, Nashville: Abingdon, 1979²; Rodney Whitacre, *Johannine Polemic. The Role of Tradition and Theology* (SBL Dissertation Series, 67), Chico, CA: Scholars Press, 1982; Craig A.Evans, *Word and Glory. On the Exegetical and Theological Background of John's Prologue* (JSNTSup, 89), Sheffield Academic Press, 1993 (esp.pp.168-172).

4 Martyn, "Glimpses into the History of the Johannine Community", in *L'Évangile de Jean. Sources, rédaction, théologie* (BETL, 44), edited by M.de Jonge, Leuven University Press, 1977, pp.149f, compares the Fourth Gospel to an archaeological tell, which contains different strata relating to different periods in the history of the one site. Similarly C.H.Talbert, *Reading John*, London: SPCK, 1992, pp.62f, points out that a Gospel (unlike an occasional letter) would contain material relating not only to the present concerns of the community, but also to past concerns as well, material which has become 'fossilized' in the tradition of the community.

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focus on an aspect of John's Gospel which most likely relates to this earlier stage in the community's history, when it was concerned to defend the messiahship of Jesus and to convince other Jews to believe.

We may begin with the Johannine account of the 'triumphal entry' (John 12:12-19), since this provides a clear illustration of the aspect of John we are studying. John here is dependent on an early tradition which is found also in the Synoptics⁶. The material probably for the most part reflects an earlier stage in the history of the Johannine community, since it mirrors the Synoptic accounts and is linked with the traditional understanding of messiahship found in Judaism and early Christianity, without any trace of the later 'high' christology. There is almost universal agreement that John is emphasizing Jesus as king and Messiah, and that as such he is a figure who comes in peace rather than as a warrior, an emphasis which is also present in the Synoptic versions, but which is perhaps enhanced in the Johannine version by presenting Jesus as mounting a donkey *in response to* the acclamation of the crowds⁷. In the Synoptics the whole event could appear to have

5 See R.T.Fortna, *The Fourth Gospel and its Predecessor*, Edinburgh: T&T Clark, 1988, pp.225-234 (Fortna is clearly right to discern differences between material formed at earlier and later stages of the community's history; whether the material was actually written down at the time when it was formed, or was crystalized as part of the tradition but written down much later, is another matter); John Ashton, *Understanding the Fourth Gospel*, Oxford:Clarendon Press, 1991, pp.246-251; Jack T.Sanders, *Schismatics, Sectarians, Dissidents, Deviants. The First One Hundred Years of Jewish-Christian Relations*, London: SCM Press, 1993, pp.40-46.

⁶ 6 Cf. C.H.Dodd, *Historical Tradition in the Fourth Gospel*, Cambridge University Press, 1963, pp.152-156.

⁷ C.K.Barrett, *The Gospel According to John*, London: SPCK, 19782, p.416; R.E.Brown *The Gospel According to John*.

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been arranged, with Jesus' entry on a donkey and the shouts of the
crowds having been the result of a pre-arranged plan to acclaim
Jesus as the Messiah and king of Israel, whereas in John the
spontaneous acclamation of the crowds triggers a response from
Jesus designed, if anything, to dampen any nationalistic fervour
that might be present.

The most distinctive aspect of the Johannine form of this
pericope is the author's note that "At first his disciples did not
understand all this. Only after Jesus was glorified did they realize
that they had done these things to him" (12:16)⁸. Most
commentators have not shed much light on this verse: while they
note the theme of scripture being fulfilled, something which
obviously would have been an important aspect of the apologetic of
any Jewish Christian group, this observation applies equally well to
the Synoptic versions of the incident and does not explain the
distinctive Johannine form. For what reason should the author
emphasize the failure of the disciples to realize what they had
done⁹ to Jesus? To suggest that this is simply a historically
accurate record of what actually occurred does not solve the
problem: John's Gospel emphasizes throughout Jesus' fulfilment of

Vol.1: I-XII (Anchor Bible, 29), New York: Doubleday, 1966,
pp.459-462; Barnabas Lindars, *The Gospel of John*, London:
Marshall, Morgan and Scott, 1972, pp.420-423; G.R.Beasley-
Murray, *John* (Word Biblical Commentary, 36), Dallas: Word,
1987, p.210.

8 As R.E.Brown notes, *op.cit.*, p.461, "There is nothing in
the Synoptics resembling John xii 16".

9 Brown (*op.cit.* p.458) notes that in John's account the
disciples do not actually do anything to Jesus. He notes the view of
Bernard that the evangelist may have the Synoptic account in mind,
but prefers the suggestion that the evangelist's words 'they had
done' simply denote a passive meaning: 'these things were done to
him'.

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various Old Testament prophecies, Jewish feasts and the like, and thus for the author to emphasize the disciples' failure to recognize Jesus' fulfilment of one such prophecy, while in the process of emphasizing the obvious nature of that fulfilment for all to see, would not appear to add anything to the author's argument. Fortna thus considers that "The Evangelist strangely blunts the immediacy of this story" by his addition in v16¹⁰, and Barrett goes so far as to write that, in suggesting that the crowds could perceive the Messianic implications of what Jesus did while his disciples could not, "The narrative is really self-contradictory"¹¹. However, G. H. C. MacGregor has offered a plausible explanation of the function of this verse in the context of the Fourth Gospel. He thinks that this verse was "added apparently...in order to emphasize the fact that the Messianic entry was not stage-managed by Jesus and his disciples as might appear from the Synoptic account"¹². This makes good sense of the differences between the Johannine account and that found in the Synoptics: whereas in the Synoptics one could get the impression that Jesus has already arranged for a donkey to be available for this event, in John Jesus is said simply to have 'found' a donkey (12:14), and the disciples are said to have been unaware until later that what transpired was actually the fulfilment of prophecy. In the context of the Johannine conflict with 'the Jews', this reading provides a plausible background against which to interpret this rather peculiar verse, and also other aspects of the Johannine portrait of Jesus as Messiah. The Jews, it may be

10 Fortna, *op.cit.* p.147.

11 Barrett, *op.cit.*, p.419. D.A.Carson seeks to avoid the contradiction which Barrett detects by referring the 'these things' to the *nature* rather than the *fact* of Jesus' kingship (*The Gospel According to John*, Leicester: IVP, 1991, p.434).

12 MacGregor, *The Gospel of John*, London: Hodder and Stoughton, 1928, p.263.

suggested, dismissed Jesus as a deceiver¹³, someone who was no different from other figures of the time who had made messianic claims for themselves. That this is a likely scenario is supported by the indicators that there were in fact quite a number of what we may broadly class as 'messianic claimants' in this period who appeared on the scene, 'claiming to be somebody'¹⁴. That Jesus would be dismissed by Jewish opponents as simply another such figure seems *a priori* likely.

If the Johannine Christians were concerned to respond to accusations of this sort, we should expect to find this reflected elsewhere in the Fourth Gospel as well, and this is in fact what there is. In the Fourth Gospel we find a definite emphasis on Jesus being one who does not seek honour for himself, but for his Father (John 8:49f,54); indeed it is God who honours Jesus (5:41-44). The reference in this latter passage to others who 'come in their own name' is regarded by many as a reference to 'false messiahs'¹⁵, a reading which makes good sense in the setting which is generally thought to lie behind the Gospel of John, and which lends further

13 This accusation occurs frequently in the rabbinic material concerning Jesus (cf. e.g., *b.Sanh.43a*), and more importantly for our purposes in John 7:12,47.

14 The phrase is from Acts 5:36f, where Jesus is also related to such figures. For a review of some of the evidence that such accusations were brought against Jesus, cf. William Horbury, "Christ as brigand in ancient anti-Christian polemic", in *Jesus and the Politics of His Day*, edited by E.Bammel and C.F.D.Moule, Cambridge University Press, 1984, pp.183-209, esp.191f,194. See also the many descriptions of such figures found in Josephus (e.g., *Ant.17; 20.97-99,171f*, and throughout his account of the Jewish War), and the useful discussion in Richard A.Horsley and John S.Hanson, *Bandits, Prophets and Messiahs. Popular Movements in the Time of Jesus*, Minneapolis: Winston Press, 1985.

15 So e.g. Brown, *op.cit.*, p.226; Carson, *op.cit.*, p.265.

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support to our argument above. In John 6:15 there is a reference to the desire of the crowds to make Jesus king by force; Jesus' response is not to accept their wishes, but rather to 'flee'.¹⁶ In a similar vein is John 18:36, where Jesus testifies before Pilate that he is a king, but one whose kingdom "is not of this world". This latter passage has recently been interpreted by David Rensberger¹⁷ as an apologetic aimed at proving to the Romans that Christianity was not a political threat. This is not impossible, but appears unlikely; The Fourth Gospel does not in any other passage appear to be concerned with the Romans for their own sake. Pilate is the only figure to make an appearance in the Gospel of John who is clearly a Gentile, and he is presented as relatively uninterested in what are to him simply irrelevant Jewish matters. It thus seems more likely that Jesus' affirmation to Pilate bears a message relevant to the conflict with 'the Jews', a supposition which appears to be confirmed by other elements of the Johannine presentation of this material.

The Jewish leaders are presented as the ones who are concerned with what the Romans will think: they classify Jesus as simply another Messianic pretender and conclude that, if they do not take fast action, the Romans will come and take away from them their temple and nation (John 11:48). Yet the Jews had, by the

16 Nestle-Aland²⁶ prefers the reading ἀνεχώρησεν, since the reading φεύγει has little manuscript support. However, Barrett, *op.cit.*, p.278; Brown, *op.cit.* p.235; and G.R.Beasley-Murray, *John*, Dallas: Word, 1987, pp.83f, and numerous other commentators accept 'fled' as the most likely original reading, which would have been softened to 'went away'. Whichever reading is accepted, the meaning is clear enough.

17 *Johannine Faith and Liberating Community*, Philadelphia: Westminster Press, 1988, pp.87-89,96-98; a similar view is taken by Carson, *op.cit.*, p.594. See also the recent work by Richard J.Cassidy, *John's Gospel in New Perspective. Christology and the Realities of Roman Power*, Maryknoll, NY: Orbis Books, 1992.

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time John was written, followed other leaders down the road to destruction which ended in the events of 70 C.E. In contrast to such figures, whom even many Jews would condemn, Jesus is presented in the Fourth Gospel as one who had not been perceived to be a threat by the Roman governor, Pontius Pilate (18:38; 19:12). In other words, John argues that the Jews rejected their true king — one who did not seek after a worldly kingship but who had been chosen by God — for the sake of political expediency, something which had in fact been unnecessary¹⁸. The key point thus appears to be the one we have found to underlie many other parts of the Fourth Gospel: Jesus is not a Messianic pretender like other purely political figures of the time.

It may be suggested that this approach also sheds light on John 10:8. This verse is fraught with textual difficulties which do nothing to expedite interpretation¹⁹. Attempts to read it as a reference to all previous leaders of the Jewish nation can hardly be correct, since there is no indication in the Gospel that Old Testament kings and leaders were not in fact sent or approved by God. Even Moses, who is contrasted with Jesus throughout the Gospel, is still regarded as a positive figure, though one subordinate to Jesus. Thus Barrett²⁰ regards the 'others' who came to be

18 And, going even further, they had very possibly contradicted the words of their own Passover liturgy and denied God himself: they asserted that they "have no king but Caesar", in contrast with the words of the Passover haggadah, "We have no King but thee, O Lord", words which may well have been used in John's time.

19 The omission of πάντες by a few manuscripts probably reflects a conviction that this would imply even OT figures. Nestle-Aland²⁶ includes πρὸ ἐμοῦ in the text, but in square brackets. See the discussion in Brown, *op.cit.*, p.386; Beasley-Murray, *op.cit.*, pp.164f.

20 *Op.cit.*, p.371; so also Bernard, cited by Lindars, *op.cit.*, pp.358f.

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Messianic pretenders, and in this he is almost certainly correct. The imagery of the shepherd is used in the Old Testament for the king²¹, and the term *λησται* (in 10:8) would also be a poignant allusion to the Messianic claimants and political activists of the period, since the term is frequently found in Josephus to designate such figures in a disparaging way. One plausible explanation for the lack of 'before me' in many manuscripts may have been a desire, whether on the part of the author or of a later copyist, to allow this verse to include also such figures who appeared after the time of Jesus²². Thus here too we find Jesus contrasted with other personages of the time who made messianic claims.

The same issue may be reflected in John 7:3-10. In this passage, Jesus' brothers urge him to go up to Jerusalem to the feast because they presume he desires to be a public figure. This assessment of Jesus and his work is classed by the author as unbelief (7:5). Jesus does go up to the feast, but *in secret*. The fact that the author repeats the very words (*ἐν κρυπτῷ*) which were used by Jesus' brothers implies that Jesus has not been correctly understood by them, i.e., he does not desire to become a public figure of this sort. He is not a seeker of fame and honour, and for this reason he is frequently found working in secret, keeping out of sight. This emphasis on Jesus frequently keeping out of the public eye may also relate to the theme, which is so prominent in the Gospel in its present form, of the desire of the Jewish leaders to kill Jesus. However, this motif appears to have a prehistory in which at least part of its importance was in combating the idea that Jesus is simply a messianic pretender, one who is primarily a political figure and who desires power and glory for himself. Jesus will eventually be exalted ('lifted up') through his death and

21 It is also applied to God, but one should not read too much into this, since this appears to be the application of a well-known metaphor for kingship to God as Israel's true king.

22 Perhaps Bar Kochba was particularly in mind, if the verse was placed in this form after the second Jewish revolt.

McGrath, **Uncontrived Messiah...?**, *IBS* 19, Jan. 1997 resurrection, according to the plan and purpose of God, and, ironically, the Jews will help to accomplish this through the crucifixion. Jesus emphatically does not seek his own glory, but the Father who sent him will 'lift him up', the passive form indicating action by God.

Another passage which appears to be related to our theme is John 1:31 where John the Baptist emphasizes that he himself did not know Jesus prior to his baptism. Many commentators emphasize that the meaning here is not that John had never met Jesus, but that he did not know or recognize him as the coming one prior to the Baptism²³. This may be correct, but it remains to be explained why John should bother to mention this fact. John Ashton²⁴ relates this to the tradition preserved in Justin's *Dialogue with Trypho* (8:4), where his Jewish interlocutor is presented as saying: "Even if the Messiah is already born and in existence somewhere, he is nevertheless unknown; even he himself does not know about himself, nor does he have any kind of power until Elijah comes and anoints him and reveals him to all". This suggestion, however, appears to flounder on the fact that in the Fourth Gospel John the Baptist emphatically denies that he is Elijah (John 1:21). A more plausible explanation of the function of this statement may be proposed by following the suggestion of MacGregor in connection with the first passage we considered: Jesus, it is emphasized here, did not make his appearance at Jordan through prior arrangement with the Baptist, nor was the Baptist's testimony the result of a prior consultation or plan. Rather, the author of this passage asserts, John did not expect Jesus to be the Messiah, and perhaps did not even know him before the baptism²⁵,

23 So e.g. MacGregor, *op.cit.*, p.30; Barrett, *op.cit.*, p.177; Carson, *op.cit.*, p.151; See also Brown, *op.cit.*, p.65.

24 *Op.cit.*, p.305. See also Barrett, *op.cit.*, p.177.

25 Of course, in John's Gospel the same event is described as in the Synoptics, but without any mention of Jesus being baptized by John. This omission is presumably also motivated by apologetic

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and made these startling assertions about Jesus only because he had received revelation from God concerning him.

Finally, we may consider the Johannine account of the resurrection of Jesus. We have evidence elsewhere in the New Testament, in Matthew 28:12-15, of what was apparently one of the earliest accusations of the Jews concerning the Christians' claims that Jesus had risen from the dead, namely the suggestion that the disciples had stolen the body. Matthew's solution is to describe these accusations as false, a scheme concocted by the Jews in order to discredit the testimony of the Christians. In the Fourth Gospel we perhaps find indicators that a similar issue was important to the Christians which produced it. For one prominent element in the Synoptics is missing from John, namely predictions that Jesus will rise from the dead. We do have references to the Son of Man being 'lifted up', a reference to his coming death and subsequent exaltation, but not giving any clear indication that anything along the lines of a physical resurrection will be a part of this process. In John 2:19 we have Jesus' promise that he will 'rebuild this temple' in three days, a saying which was understood only with hindsight to apply to the resurrection (2:22). Even after the event we do not find the disciples recalling words of Jesus which foretold these things (cp. Luke 24:6-8), but rather they are described as slowly coming to understand what had happened in light of the scriptures (cf. John 20:9). It is very possible that John is in fact close to the historical reality here: he expresses a conviction that Jesus had foreseen or foreknown these things, and yet also indicates that Jesus had said nothing which unambiguously promised that these things would happen. Yet the author's emphasis here on the failure of the disciples to understand these things bears such a resemblance to the passages which we have discussed above that it seems difficult to avoid the conclusion that here too the evangelist wishes to avoid presenting the events in any way that might give the impression that they had been staged. Thus it is the followers of Jesus

aims, presumably against a continuing group of disciples of John the Baptist who claimed the supremacy of the Baptist over Jesus.

McGrath, **Uncontrived Messiah...?**, *IBS* 19, Jan. 1997 themselves, those who first found the empty tomb, who are fearful that someone has stolen the body. It is only after the event, after some reflection, that they understand that these things happened to Jesus because they had in fact been foretold in scripture. Far from having staged these events in order to be able to claim that Jesus had fulfilled certain Old Testament promises, they found these occurrences traumatic, and only came to see them in relation to the scriptures after the event.

It would seem that we have uncovered an important aspect of the Johannine community's apologetic, during at least one stage in the history of its conflict with those Jews who did not believe Jesus to have been the Messiah. These Jewish objectors, it appears, dismissed Jesus as simply one of the many figures to appear on the scene during these troubled, stress-filled years of Israel's history. In their view he claimed to be someone important, sought honour and power for himself, before finally meeting the end which awaited so many of those who sought to profit from the social and political unrest which plagued Israel in those times: death by crucifixion. The Johannine Christians responded by seeking to present Jesus as one who did not seek after his own honour, but only that of his Father, God. They stressed his secrecy and his rejection of any attempts to regard him as a figure of merely political significance. Jesus had been 'lifted up' by God in direct connection with his role as a *suffering* Messiah, the Son of Man who is 'lifted up' to be crucified. We see here a connection with the very early discussion within Christianity of the significance of Jesus' death. It is precisely as Israel's king that Jesus suffers, representing the nation in accepting God's judgement upon it. Again, what may have been in the mind of the historical Jesus, and what prior arrangements may have been made for various events in his career, it is impossible to say from a study of the Fourth Gospel alone. However, we can say what John thought of the suggestion that Jesus had aspired to greatness and, believing himself to be the Messiah, had orchestrated the fulfilment of Old Testament prophecies in order to convince the world that he is who he says he is. As far as the Johannine Christians were concerned, Jesus lived as he did, in

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fulfilment of prophecy, for no other reason than that it was the plan and purpose of God that it should be so.

It would thus seem that we have uncovered an important aspect of Johannine apologetic at one stage in the community's history, and probably quite an early stage at that. However, it is important to ask also whether this study accomplishes anything beside the satisfaction of historical curiosity. I believe that it does. For one thing, we have seen a tendency to make certain material in the tradition more ambiguous, such as in the case of the predictions of the resurrection. This is important, since there is a tendency in the practice of redaction criticism for the assumption to be made that 'more fully developed' material, i.e., material which is more explicit or is expressed more clearly, is later: this is often the case, but in some instances an author or editor may have had reasons to make a clear statement appear more ambiguous²⁶. What are we to make of the unambiguous prediction found in Mark 8:31 that the Son of Man will be put to death, and then on the third day rise again, in comparison with the lack of any unambiguous prediction in John?²⁷ Mark says that Jesus 'said these things openly/plainly' (Mark 8:32), which is quite different from the Johannine portrait. The main question is whether John knew a Markan type version of Jesus' predictions, but chose to play them down, or alternatively whether John preserves more accurately what actually was the case,

26 One may note the suggestion of John P.Meier, *A Marginal Jew. Rethinking the Historical Jesus. Volume I*, New York: Doubleday, 1991, pp.128-134, that the author of the *Gospel of Thomas* knew the Synoptics, but made the meaning of the parables less explicit because he wished to present a collection of mysterious, enigmatic sayings. On the tendencies in the development of tradition see especially E.P.Sanders, *The Tendencies of the Synoptic Tradition* (SNTS Monograph Series, 9), Cambridge University Press, 1969.

27 As we have already noted above, John 2:29-22 is far from unambiguous.

McGrath, **Uncontrived Messiah...?**, *IBS* 19, Jan. 1997 and was driven by apologetic reasons *not* to follow the path taken by Mark and the other evangelists in portraying Jesus' foreknowledge. It may prove impossible to settle the issue, but what is most important is the corollary which we have already noted, namely that apologetic motives can frequently explain not only the tendency to make Jesus' predictions more explicit, in order to demonstrate his power and supremacy through his detailed foreknowledge, but also to make the tendency *less* explicit, in order to avoid any suggestion that Jesus was staging or manipulating events in order to accomplish his purposes.

More importantly, an understanding of this earlier phase may help us to understand some of the reasons why later Johannine christology took the form that it did. As is well known, in the community which produced the Fourth Gospel a higher christology developed²⁸ which regarded Jesus as a pre-existent divine figure. The author of the Gospel is concerned to present Jesus (or the pre-existent Word or Son of Man) as one who is rightly called God, and who is thus worthy of the exalted status and divine honours given to him by the Johannine Christians. Yet we still find within the Fourth Gospel in its present form an emphasis on Jesus' humility and his dependence on the Father. This combination of divinity and subordination is an aspect of the Gospel which has intrigued scholars for a long time. May it not be suggested that, when certain tendencies in the community's christology pulled them towards a high christology, other elements in their tradition, which were part of this earlier apologetic, could not be eliminated, and thus material with quite different emphases were combined to produce the distinctive and unusual Johannine portrait of Jesus? If such an explanation is plausible, then our study may be of more than merely historical interest after all: it may help to explain not only what was important at an early stage in the history of Johannine Christianity,

28 An explanation of this development cannot be given here, but will have to await a future study.

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but also why the Fourth Gospel and its christology eventually took the form that they did.

James F. McGrath

**THE STRUCTURE OF HEBREWS
AND OF 1ST AND 2ND PETER**

The Rev. Dr. George K. Barr

The study of the scale relationships¹ between literary works introduces a new term - 'scalometry'. An introduction to this procedure and a scalometric analysis of the Pauline epistles were given in *Irish Biblical Studies*, Vol. 17, pp. 22-41. In that paper, the 'prime patterns'² of the Paulines were identified and were seen to be related through sharing a scaling system which has not thus far been found in other authors' work. When the cumulative sum graphs of Hebrews and 1 and 2 Peter are compared with those of the Paulines, it is evident that we have entered a different environment. It is also apparent that these three epistles have some characteristics in common. There is a similarity in texture, even though the patterns do not match over the complete works. This was noticed by the author in 1965, but only in recent years was the opportunity found to undertake detailed graphical comparison, section by section.

¹ Barr, George K. PhD thesis, *Scale in Literature - with reference to the New Testament and other texts in English and Greek*. University of Edinburgh, 1994.

² 'Prime pattern' - a particular kind of cumulative sum graph. Sentence sequence graphs plot the sum of variations in sentence length progressively from the beginning to the end of the work or sample. While these traces may reflect the idiosyncrasies of authors, they may also be affected by interruptions, use of quotations etc. Prime patterns show an unusual consistency of form and feature, and show strong contrasts. Experience suggests that prime patterns reflect material which has been thought through and written out or dictated in one operation. Material written in several sittings, or conflated, tends to lose these contrasts. Such patterns representing 'primary' material (as against 'secondary' afterthoughts) are rare, but when they are found, can be a valuable indication of authorship.

Problems arise in relation to punctuation and embedded quotations. The punctuation given in the United Bible Societies' editions largely agrees with Souter regarding the principal stops, and the punctuation of UBS Third edition (corrected) has been generally used in this study except for one important passage. About the middle of 1 Peter there is a passage which is clearly on a larger scale than the remainder of the text. UBS3 has yielded to the temptation to divide the longer sentences, while Souter has continued the large scale treatment too far and extended it into what is clearly a small scale passage. The best balance of scale is obtained by using UBS3 generally, but following Souter in 3:8-4:6. This involves only the substitution of five colons, following Souter in each case, in place of the full stops or interrogation marks found in UBS3.

Quotations pose a problem in Hebrews. The first three chapters contain so many quotations that there is insufficient material left to gauge the scale. No attempt is made to solve this: it is accepted as an enigma and simply noted. Sometimes quotations which have been conceived at a different scale level from their new context require to be grouped, and this is necessary at Hebrews 10:5-9a and 10:30-31 where in each case three sentences of quoted material have been taken to form one word group.

One other problem remains before the graphs of the three works may be compared. Hebrews is very much longer than the other two epistles, and consequently has the scope to develop much more graphical detail. This appears as a finer and more complex saw-tooth pattern which obscures the underlying structure. It may be described as 'noise'. One solution to the problem is to take the sentences in batches of three and to use the average length of the sentences in each batch as one 'sentence unit'. This brings the scale of the detail in the graph of Hebrews more or less into line with the scale of the other two epistles. The graphs of Hebrews in Figs. 2 and 5 (see graphs at end of this paper) show the effect of doing this. Another more sophisticated technique called 'SuperQsums' is also introduced below.

With these modest adjustments, the prime patterns may now be identified. The whole of Hebrews, in fact, provides a prime

Barr, **Hebrews and 1st and 2nd Peter**, IBS 19, Jan. 1997 pattern. In 2 Peter, the form which 3:14-18 takes on the graph suggests that it is an addendum: otherwise the whole of the material provides a prime pattern.

1 Peter is more complex, as there is conflict between the prime pattern and the layout of discourse units. Both William Schutter³ and Lauri Thurén⁴ correctly take 1:13-2:10 to be one discourse unit, yet the graph shows a prime pattern beginning at 2:1. Experience with other prime patterns suggests that the author of 1 Peter began by writing 1:1-25 which concludes 'That word is the good news which was preached to you', and then laid the work aside for a time. On resuming his writing, he was unable to pick up the pattern established in the first chapter, and began a new prime pattern which ends with the 'Amen' at 5:11. The greetings of 5:12-14 (Peter's subscription?) appear as an addendum to the pattern. That a double beginning was made is confirmed by the graph in Fig. 1 which shows the first beginning at Chapter 1 with the new beginning of Chapter 2 superimposed (Souter was used for this small exercise). The patterns of the two beginnings are similar, but the scale of the second beginning is smaller than that of the first thrust, the average sentence lengths of the two beginnings being 36.9 words and 31.1 words respectively. (That is why the dashed line on the graph indicating the continuation of Chapter 1 does not match the superimposed dotted line which represents the second beginning at Chapter 2 - they are drawn to different average sentence lengths.)

The three prime patterns are shown in Fig. 3, scaled to a common base and superimposed. It can be seen that the traces follow each other except at the anomalous beginning of Hebrews where quotations disrupt the trace. What is important in these comparisons is the similarity of motif rather than the shape or size of features, although the overall proportions are also significant.

³ Schutter, William. *Hermeneutic and Composition in 1 Peter*. Tübingen: Mohr, 1989.

⁴ Thurén, Lauri. *The Rhetorical Strategy of 1 Peter*. Åbo Academy Press, 1990.

The graphical motifs have been separated and compared in Fig. 4; this is simply a graphical separation and is unrelated to content.

A striking feature is the large scale section which lies in the centre of each prime pattern: this causes the steep rise in each graph in the centre section. From a graphical point of view, the changes in scale at the beginning and at the end of each of these large scale sections are remarkably consistent in that they occur at almost the same relative position in each prime pattern. As these works are of very different lengths, the positions of these changes are determined as a percentage of the text measured from the beginning of the prime pattern. They are placed as follows:

| <u>EPISTLE</u> | <u>1ST</u> <u>CHANGE</u> | <u>2ND</u> <u>CHANGE</u> | <u>CENTRAL</u> <u>SECTION</u> | <u>PRIME</u> <u>PATTERN</u> | <u>TOTAL</u> <u>WORDS</u> |
|----------------|-----------------------------|-----------------------------|----------------------------------|--------------------------------|------------------------------|
| 1 PETER | 515 (41.9%) | 853 (69.3%) | 3:8-4:6 | 2:1-5:11 | 1230 |
| 2 PETER | 443 (44.5%) | 648 (65.1%) | 2:4-16 | 1:1-3:13 | 995 |
| HEBREWS | 2132 (43.0%) | 3269 (66.0%) | 8:1-10:25 | 1:1-13:25 | 4953 |

When it is considered how varied the sentence sequence patterns found in one author's works can be, it is remarkable that these dramatic scale changes should be positioned so consistently in proportion to the lengths of the respective epistles. No comparable pattern has been found in a close examination of the graphs of over half a million words from six English authors, one Latin and four other Greek authors. Yet here the pattern occurs three times comprising just over 7,000 words.

It is noteworthy that the central section in each case contains the kernel of the epistle's message. In 1 Peter it begins to\ de\ te\loj, 'finally', marking the change in scale (compare to\ loipo/n, 'finally' which serves the same purpose in the Paulines) and the theme might be entitled 'Partakers of the Sufferings of Christ'. In 2 Peter, the core of the message in 2:4-16 is 'The Judgement of God'. In Hebrews the main thrust begins at 8:1 with the reference to Jesus as high priest, seated at the right hand of the throne of the Majesty, and it ends with a picture of the exalted

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Christ, his work of reconciliation accomplished, awaiting the final
Triumph. This leads to the climax of the appeal in 10:19-25.

Further light, however, is shed on the central sections when the text-linguistic analysis of Hebrews by George H. Guthrie⁵ is combined with the scalometric analysis. Guthrie identifies cohesion shifts where changes in the structure of the texts occur, and also locates *inclusio* which are marked by characteristic phrases and vocabulary at the opening and at the closing of each inclusion. In Hebrews there is a major central inclusion (4:14-16 to 10:19-23 concerning the Priesthood of Christ) which embraces two embedded inclusions. These are shown on the graph in Fig. 2 and it is seen that the latter embedded inclusion corresponds to the large scale section of the scalometric analysis beginning at 8:1. The text marking the closing of an inclusion does not necessarily come right at the end of the discourse unit, and in this case the text runs on to the next cohesion shift at 10:39/11:1 where a new subject (Faith) is introduced. These two embedded inclusions are similar in length (35 and 30 sentences) but different in scale with average sentence lengths of 29.69 and 37.9 words respectively.

This pattern is compared with the patterns found in 1 and 2 Peter in Fig. 5. It is seen that there is a major inclusion in 1 Peter which opens at 2:21 with 'For to this you have been called, because Christ also suffered for you, leaving you an example, that you should follow in his steps. He committed no sin...' and closes at 4:1 with 'Since therefore Christ suffered in the flesh, arm yourselves with the same thought, for whoever has suffered in the flesh has ceased from sin...' (RSV). A break in this inclusion occurs at 3:8 and is marked by *to\ de\ te/loj*. Again, like the major inclusion in Hebrews, the two parts of the inclusion in 1 Peter are similar in length (six sentences in each) but differ in scale with average sentence lengths of 33.83 and 46.33 words. At the end, the sense runs on to the next cohesion shift at 4:6/7 which is placed on the graph in the same relative position as the shift in Hebrews.

⁵ Guthrie, George H. *The Structure of Hebrews*. Leiden: Brill, 1994.

The prime pattern of 2 Peter shows similar features. The large scale section detected in the scalometric analysis (2:4-16) might also be considered to be an inclusion. The thought of the passage is enclosed within the reference at 2:4 to fallen angels being consigned to hell and kept chained in darkness, and the reference at 2:17 to the false prophets being consigned to eternal darkness. In this case the passage corresponds to the latter embedded inclusion in Hebrews and to the latter half of the major inclusion in 1 Peter (from to\ de\ te\ loj to the end of the inclusion). The text prior to the passage (marked with a dotted line in the graph of 2 Peter in Fig. 5) has the same topographical features as the corresponding parts of the other epistles and shows a similar difference in average sentence length.

Comparison of the three graphs in Fig. 5 shows that all three epistles have central passages with similar scaling features and with minor variations, but displaying a common topography. The remaining portions of the graphs also show common features, except that the initial part of the graph of Hebrews is distorted by quotations for which allowance must be made. There is little doubt that these are indeed prime patterns, but different in form and character from those found in the Pauline epistles. There is great variation in the length of these works; the graphs in Fig. 6 show their relative sizes, but the proportions are maintained. The Pauline examples shown in Fig. 7 also maintain a common shape over considerable differences in length.

Earlier, a method of dealing with the differences in the traces of works conceived at different scales was given. This involved grouping the sentences of longer works in batches of three. An alternative way of dealing with this problem was devised resulting in the development of SuperQsums. (Apologies are offered for a term which smacks of jargon, but no other label could be found which so neatly describes this kind of graph.) An ordinary cumulative sum graph smoothes out the contrasts between the lengths of successive sentences and reveals something of the underlying structure, but in the longer works, fine saw-tooth detail persists in the graphs, making direct comparison with shorter works difficult. In an ordinary cumulative sum graph, the cumulative sum

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of variations in sentence length is plotted against the sequence of sentences. SuperQsums take the process a stage further. In a SuperQsum, the cumulative sum of the cumulative sums of variations in sentence length is plotted against the sequence of sentences. This has the effect of smoothing out all the saw-tooth patterns and reveals the basic structure of the work. It becomes possible to compare directly the sub-structures of works which are as different in length as Romans and Philemon. The ordinary cumulative sum graphs are used to compare size, shape and proportion, and the scales of each graph must be precisely and scientifically related. In employing SuperQsums, the aim is rather to compare the topography (the nature of the essential features) rather than the precise shape or size.

SuperQsums are not an infallible guide to authorship - they reflect decisions made concerning punctuation etc. - but it is useful to compare the SuperQsums of prime patterns produced by different authors. Four sets of SuperQsums are shown in Fig. 8. The selection from Seneca's works comprises six letters in which there is clear contrast between an opening narrative portion and a closing reflective portion. Seneca shows great variety in structural pattern but a general trend is apparent in this group of examples. These may be compared with the prime patterns of the thirteen Pauline epistles which have a large scale opening portion, sometimes theological, and a small scale closing portion, sometimes ethical. These Pauline prime patterns comprise 67% of the text of the corpus, or 88% if the small topics in the Corinthian correspondence are omitted. The Ignatian SuperQsums also reflect the strong prime patterns found in some of his letters. It is against these groups of epistles, which to a greater or lesser degree have prime pattern characteristics, that the SuperQsums of Hebrews and the Petrine epistles should be viewed. Obviously these three epistles (Fig. 8) lie close together although they are on very different scales. The sub-structures of Hebrews and 1 Peter might be said to be scaled-up versions of the sub-structure of 2 Peter.

Examples of this kind of correspondence at different scale levels have hitherto been found only in works which had a common author, and this possibility must be taken seriously in the case of

Hebrews and 1 and 2 Peter. In a short article it is not possible to explore the implications of this proposal in detail, but a few points may be noted. There is no obvious intention on the part of the author(s) of 1 and 2 Peter to imitate Hebrews in terms of style, vocabulary or thought - or vice versa. The quality of Greek varies from 'among the best in the New Testament' in the case of Hebrews (Kümmel) to 'cumbrous and obscure' in the case of 2 Peter (Moffatt). It should be appreciated, however, that a writer, ancient or modern, may embrace a wide range of styles and vocabulary depending on circumstances. In my own experience, I have written a theological essay in careful English which ended with a poem, as that seemed to be the appropriate way to end the work. In contrast, I have written hundreds of technical reports giving advice to statutory bodies; these were written in a different style with different vocabulary. I have also written letters to defaulting contractors who had failed to keep their promises - letters which regrettably descended to the level of invective. These three classes of writing, all employing different styles and vocabulary, all with different purposes and destinations, may be compared with the excellent Greek of Hebrews, the sound Greek of 1 Peter, and the crude Greek of 2 Peter.

Some scholars would relate the content of 2 Peter to heretical systems which flourished in the second century, and are perhaps too ready to date material according to the period when a heresy was in full flower. But heresies have their roots in the failures of human nature, and the second century heresies had their roots in small but troublesome developments in the first century, in dangerous trends to which Paul and other early leaders were particularly sensitive.

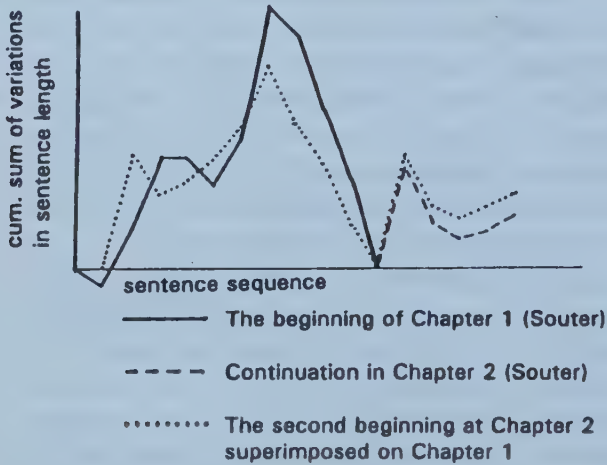
If these three epistles come from the same hand, the Biblical evidence must not be ignored, and Silvanus (1 Peter 5:12) might well be taken to be Peter's amanuensis. The material, however, has not been taken down verbatim, as was the case with the Paulines. Rather Silvanus has been given freedom to convey the thought in his own way; hence the prime pattern is that of Silvanus.

If that is indeed what happened, then we are presented with the intriguing possibility that no fewer than sixteen New Testament epistles come from the hands or minds of Paul and his travelling companion, Silvanus. This points to a close group of leaders at an early stage who argued out their faith in discussion, and whose different views are reflected in their writings. It may also raise the question whether material in the epistles of James and Jude might also reflect discussion within such a group.

The concept of the New Testament epistles being the product of a large number of unidentifiable authors has been supported by recent statistical work; Anthony Kenny stands alone in his less sceptical conclusion that 'on the basis of the (statistical) evidence in this chapter for my part I see no reason to reject the hypothesis that twelve of the Pauline Epistles (excluding Titus) are the work of a single, unusually versatile author'.⁶ The more sceptical conclusions have resulted in part from the failure of statistical studies to identify the important scale variable. Consequently, many differences which are due to the kind of variations in scale which are commonly found within the works of one author have been mistakenly attributed to differences in authorship.

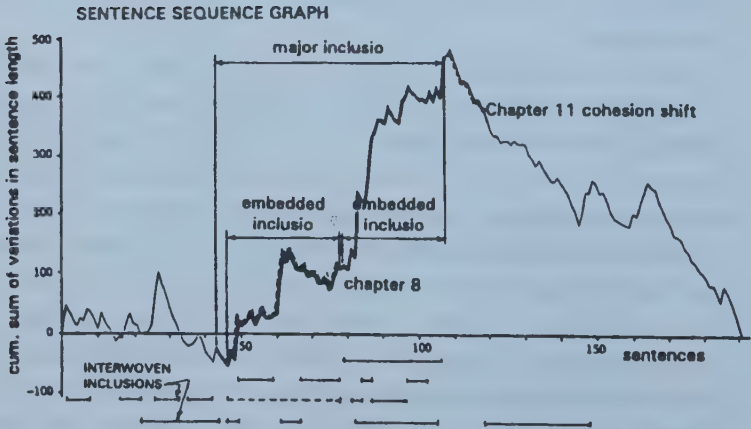
Prime patterns are produced sub-consciously, and experiment has shown that it is virtually impossible to reproduce them by conscious imitation. With regard to the patterns described above in Hebrews and 1 and 2 Peter, it is quite incredible that three different unknown authors, writing at unknown times and in unknown places, and who left behind no other identifiable works, should produce these three epistles which show such closely corresponding scaling structures coinciding precisely with identifiable discourse units relating to widely differing subjects. The scalometric evidence combined with that derived from text-linguistic analysis strongly supports the hypothesis that these three works come from the same hand. Internal evidence points to Silvanus as co-author with Peter in writing at least one of his

⁶ Kenny, Anthony. *A Stylometric Study of the New Testament* (Oxford: Clarendon Press, 1986), p. 100.



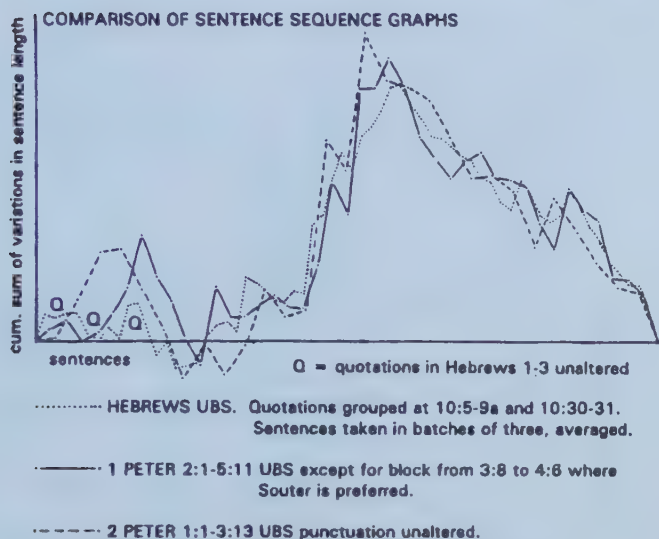
THE TWO BEGINNINGS IN 1ST PETER

Fig. 1



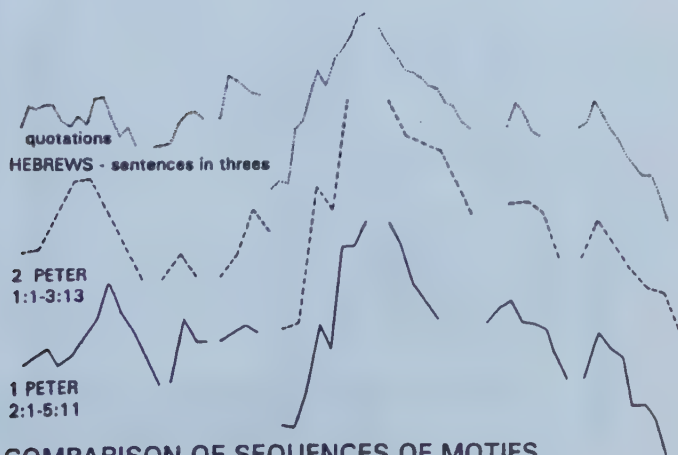
LOCATION OF MAJOR INCLUSIO IN HEBREWS
AS IDENTIFIED BY GEORGE H. GUTHRIE

Fig. 2



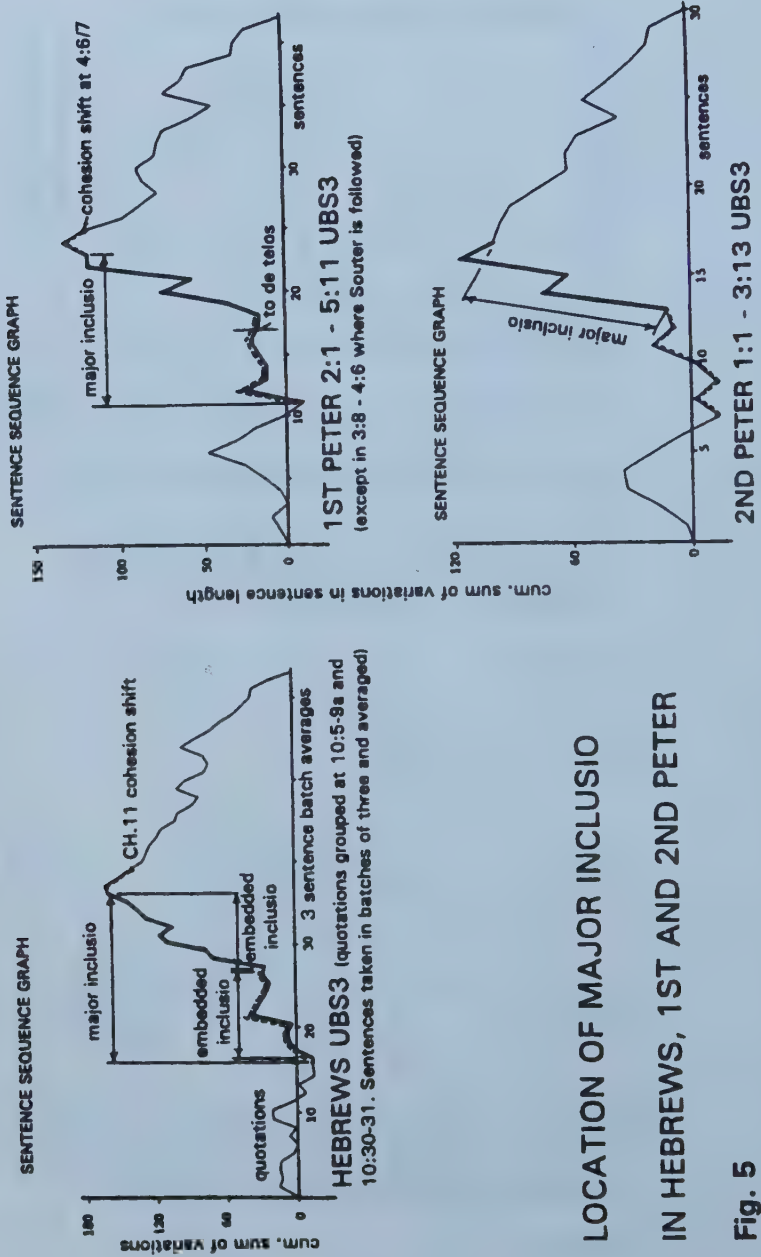
COMPARISON OF PRIME PATTERNS IN HEBREWS, 1ST AND 2ND PETER

Fig. 3



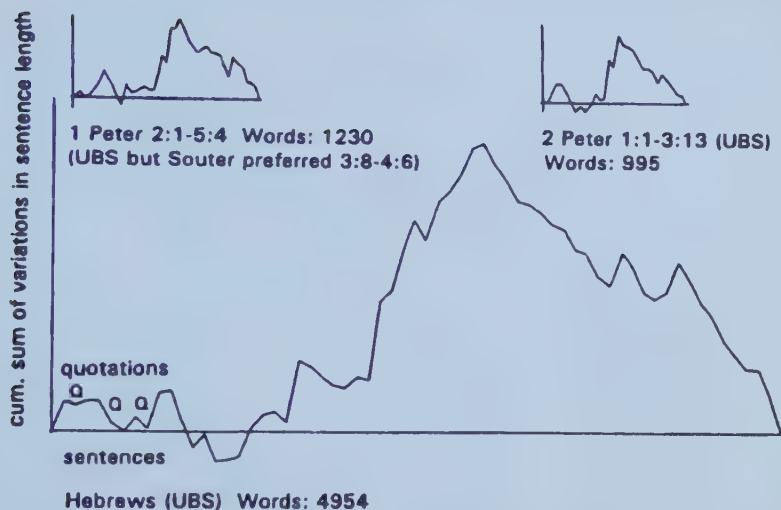
COMPARISON OF SEQUENCES OF MOTIFS

Fig. 4



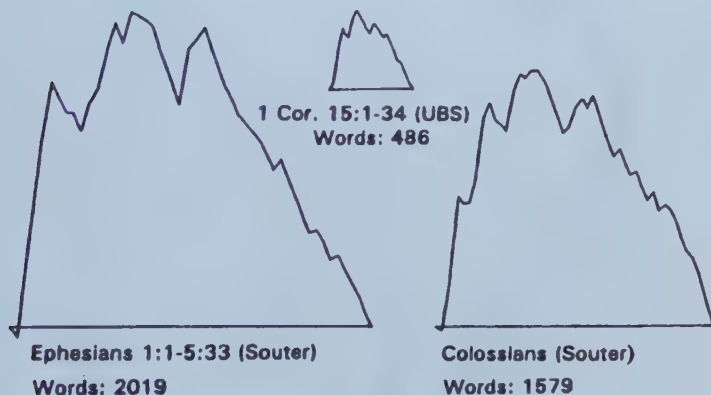
LOCATION OF MAJOR INCLUSIO
IN HEBREWS, 1ST AND 2ND PETER

Fig. 5



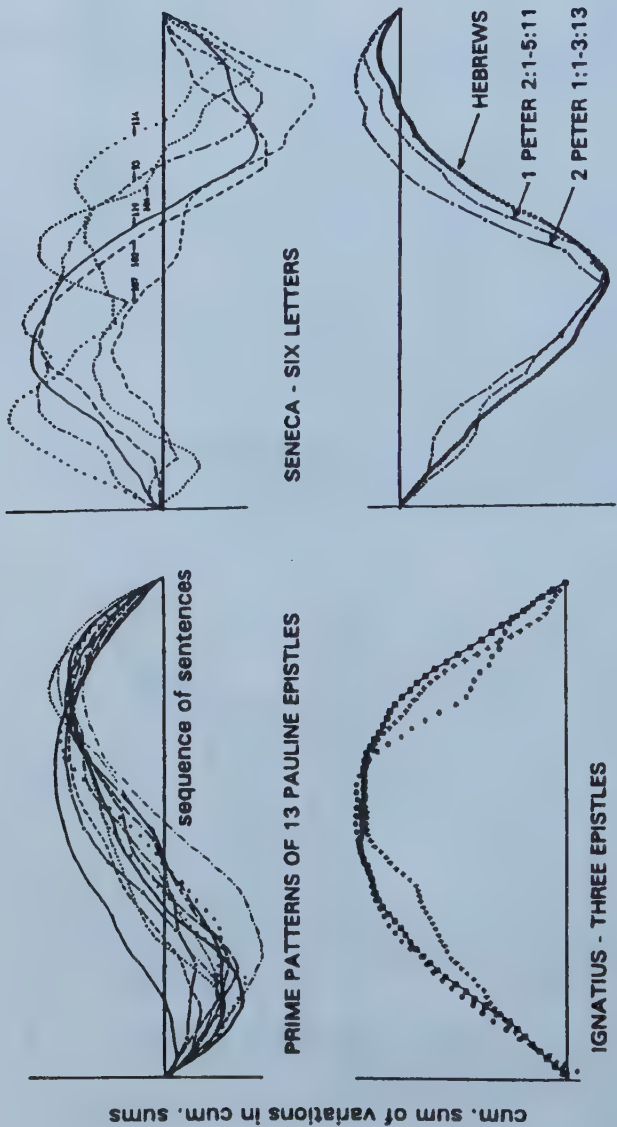
SCALING RELATIONSHIP BETWEEN WORKS - HEBREWS, 1ST AND 2ND PETER

Fig. 6



SCALING RELATIONSHIP BETWEEN WORKS - THREE PAULINE EXAMPLES

Fig. 7



SUPERQSUMS OF SEVERAL AUTHORS

Fig. 8

Barr, **Hebrews and 1st and 2nd Peter**, *IBS 19, Jan. 1997*
epistles. The patterns link the two Petrine epistles to Hebrews. It
follows therefore, that Hebrews may justifiably be considered to be
the Gospel according to Silvanus.

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MacAdam, Book Review Article, *IBS* 19, Jan 1997

FUMUS ET SPECULA: GOD AND MAMMON IN THE JESUS-BOOK INDUSTRY. A REVIEW ARTICLE BASED ON CARSTEN PETER THIEDE & MATTHEW D'ANCONA, *EYEWITNESS TO JESUS: AMAZING NEW MANUSCRIPT EVIDENCE ABOUT THE ORIGIN OF THE GOSPELS*. DOUBLEDAY, NEW YORK & LONDON, 1996. ISBN 0-385-48051-2. Pp. XI + 206. FOURTEEN PLATES. \$US 23.95. ALSO PUBLISHED IN THE U.K. AS *THE JESUS PAPYRUS*. WEIDENFELD AND NICOLSON (1996) £16.99 ISBN 0-297-81658-6.

For Bruce M. Metzger

Dr Henry Innis McAdam

"So the disciples went ahead and did what Jesus had told them to do: they brought the donkey and the colt, threw their cloaks over them, and Jesus got on."

Matthew 21:6-7

An aura of sensationalism has surrounded the subject of this book since Britain's prestigious *Times* ran a front page story on 24 December 1994 announcing that the library at Magdalen College, Oxford University possessed "... the oldest extant fragment[s] of the New Testament ..." The focus of that report was three tiny scraps of papyrus acquired through an alumnus donation in 1901. Each fragment is the size of a postage stamp, and all contain Greek lettering identified as portions of the *Gospel of Matthew*. The text appears on the front (*recto*) and back (*verso*) of the three fragments, yielding a total of six or seven dozen legible letters. The complete document was a codex or early form of a book rather than a scroll. The three scraps at Oxford now have the official designation of *Papyrus Magdalen Greek 17*.

This "Christmas exclusive" was written by Matthew d'Ancona, then a Deputy Editor of the *Times*, now an Associate Editor of London's *Sunday Telegraph*. His source for the piece was Carsten Peter Thiede, a German papyrologist who had become interested in the Magdalen College scraps during a family visit to Oxford the previous February. The *Times* reported Thiede's claim that the A.D. 180-200 date assigned to the Magdalen fragments more

than forty years ago is at least a century too late. Thiede contended that the Magdalen remnants of *Matthew* 26:7-33 were written before A.D. 70 and perhaps as early as the middle of the first century A.D., i.e. only two decades after the Roman execution of Jesus of Nazareth. Thiede's argument for that astonishingly early date, as reported in the *Times*, was based on palaeographic comparison with other documents of the first century A.D.

Just over twenty years ago Fr. José O'Callaghan argued that a similar scrap of Greek papyrus (7Q5) from the Qumran/Dead Sea material contained snippets of *Mark* 6:52-53. That has been hotly disputed, but it is now safe to say that even more heat will be generated by Thiede's claims for the Magdalen Library fragments. Since the publication of the *Times* article Thiede has set out his views for scholarly discussion in the German journal *Zeitschrift für Papyrologie und Epigraphik* Vol. 105 (1995) pp. 13-20 & Tafel IX. The volume under review expands on that for non-specialists. Both the article and the book press the claim set forth in the *Times* that the Magdalen Library fragments date from before the Roman destruction of Jerusalem (A.D. 70) and that the period in which the Gospels appeared in written form is therefore several decades earlier than previously believed.

Here is how this claim is presented in the opening chapter of *Eyewitness to Jesus*, referring back to d'Ancona's *Times* story and Thiede's article for *ZPE*:

"Not since the discovery of the Dead Sea Scrolls in 1947 had there been such a potentially important breakthrough in biblical research. Thiede appeared to have found evidence that the St. Matthew Gospel was written only a generation after the Crucifixion--or even earlier. The papyrus itself, unearthed in Upper Egypt and bequeathed to Magdalen in 1901, might conceivably have been read or handled by one of the "five hundred brothers and sisters" (1 *Corinthians* 15:6) whom St. Paul declares to have seen the resurrected Jesus with their own eyes. It was a claim that nobody with an interest in Christianity--spiritual or scholarly--could possibly ignore." (p. 3)

While d'Ancona fielded the flood of telephone calls and the correspondence generated by his piece in the *Times*, Thiede went "on the road" with his message:

"In his scholarly travels around the world [1995], Thiede discovered that his research had made an impact upon ordinary people, fascinated by the questions it posed to them about the relationship between history and faith, religion and empiricism. They were enthralled that the fragments might have been read by men and women who had walked with Jesus through Galilee and wept as the storm [*sic*] gathered above the Cross on Golgotha" (p. 4)

Ancient historians, biblical scholars and papyrologists were less "enthralled". Thiede's *ZPE* article was quickly rebutted in that same journal by Klaus Wachtel under the title: "P 64/67: Fragmente des Matthäusevangeliums aus dem 1. Jahrhundert?" (*ZPE* 107 [1995] 73-80). Reference to it is relegated to an aside (pp. 61-2) and an endnote (p. 176 note #50) in *Eyewitness to Jesus*. One can understand the haste with which d'Ancona and Thiede try to minimize Wachtel's critique. After six closely-argued pages in which Thiede's main arguments are disputed point by point (even to the discussion of comparison of specific letter-forms in the Magdalen and Barcelona fragments with those in other documents), Wachtel concludes:

"Es is in jeder Wissenschaft förderlich, sich von Zeit zu Zeit ihrer materialen Grundlagen zu vergewissern. Thiedes Argumentation für eine Datierung des P 64/67 ins 1. Jahrhundert ist jedoch als methodisch unzulänglich und sachlich falsch zurückzuweisen. Die herkömmliche Datierung in die Zeit um 200 hingegen hat sich als gut begründet erwiesen." (*ZPE* 107 [1995] 80)

Dismissing such an indictment as little more than "... a form of intellectual resistance which can not last" (*Eyewitness* p. 62), d'Ancona and Thiede then go on to assert that their book

is a response to the flood of interest in the Magdalen Papyrus. It is neither a religious tract nor an exercise in Christian persuasion. Instead, it seeks to make accessible to the general reader a major papyrological discovery and its implications for the dating of the New Testament and our knowledge of early Christianity. It seeks to bridge the gap between scientific investigation and the questions which

every thinking person must ask about the Gospels and their significance. It seeks to promote debate as much as to answer questions definitively (p. 5).

Just what is meant by this “gap” is not made clear, though I get the feeling after reading their book that it is the public at large and not the world of “scientific investigation” to whom the authors are turning for acceptance. Just what “evidence” is there that would allow them to suggest such an early date for *Matthew*? The presentation of their argument is done in a rather awkward way. Instead of following a short introductory chapter with some background information on how and when the Magdalen scraps came to Oxford, the reader must first wade through more than 60 pages on “St. Matthew and the Controversy over the Origins of the New Testament” (Chap. 2) and “Investigating the Magdalen Papyrus” (Chap. 3). Let us move ahead first to their account of how the papyrus got to Oxford: “The Discovery of a Lifetime” (Chap. 4).

The three scraps had been acquired in Luxor, Egypt by the Rev. Charles B. Huleatt, a Magdalen alumnus who served as the Anglican chaplain at Luxor from 1893-1901. Huleatt thought they dated to the third century, but the librarian to whom they were sent was advised they might be *fourth* century. That advice came from Arthur Hunt, a fellow alumnus of Magdalen and collaborator (with Bernard Grenfell, of Queen’s College, Oxford) on the then-new Oxyrhynchus Papyri project. The publication of that archive (sixty-one volumes to date) is still underway as this century draws to a close.

Huleatt never saw his *Matthew* fragments again. He and his family perished when the great earthquake of 28 December 1908 destroyed the city of Messina (Sicily) where they had resided after leaving Egypt in 1901. His bequest wasn’t given any special attention until after World War II. The first editor was Colin H. Roberts, a British papyrologist who had identified and dated the famous fragment of the *Gospel of John* (P. Rylands Gk. # 457) in the John Rylands Manuscript Library (now Rylands Library of the University of Manchester); see his *An Unpublished Fragment of the Fourth Gospel* (1935). Roberts dated the Rylands fragment to about A.D.

120 and it is still regarded by most New Testament scholars as the earliest extant segment of any Gospel.

In the early 1950s the three Magdalen fragments were brought to Roberts' attention. After researching the library's records he observed that "It is probable that there were further fragments of the same leaf since a letter by Mr. Huleatt to the Librarian [of Magdalen College] refers to purchases of fragments from the same manuscript in successive years" (*Harvard Theological Review* 46 [1953] 233). Neither Thiede's *ZPE* article nor *Eyewitness to Jesus* makes any mention of that statement and its suggestion that Huleatt had bought more than the three scraps attributed to him.

Indeed, Roberts later *did* identify another two papyrus fragments of this same Matthew codex in a Spanish publication. They had been identified as parts of *The Gospel of Matthew* (Mt 3:5; 3:15; 5:20-22; 5:25-28) and were edited by Ramón Roca-Puig, *Un Papiro Griego del Evangelio de San Marco* (1956) They are known officially as *P. Barc. Inv. # 1* and are in the possession of the Fundación San Lucas Evangelista in Barcelona. As far as I know, no connection to Charles Huleatt has ever been established. Their provenance, incredibly, is totally ignored by Roca-Puig. Thiede and d'Ancona discuss these at some length (pp. 64-70), though it is evident that Roca-Puig accepted Roberts' dating of c. 200 for *all* the Matthew codex fragments. Several attempts to connect the Magdalen and Barcelona fragments with others (e.g. with *P. Suppl. Gr. # 1120* in Paris) have so far proved fruitless.

Though Roberts could not date the Magdalen fragments with anything approaching precision, he took issue with the original assigned date of third or fourth century A.D. That was because

... it has been recognized for some time that Grenfell and Hunt were unduly conservative in their dating of some early Christian papyri, largely because they accepted the assumption common to palaeographers of the last century that a manuscript written in a codex could not be earlier than the third century ... it should be remembered that when Hunt saw the [Magdalen] papyrus in 1901 relatively few Biblical papyri had been published and the development of Greek

palaeography was less clear than it has since become ... (*HTR* 46 [1953] 234-5).

Roberts then went on to conclude that "In its strong and firm lines and in spite of certain angularities the hand of the papyrus may be regarded as an early predecessor of the so-called 'Biblical Uncial', a hand 'whose peculiar style began to form towards the end of the second century'" (*ibid.* 235). It is very important to emphasize that last statement, which Roberts credits in note #7 on that page to "W. Schubart, *Griechisch Paläographie*, p. 136". The closest that Thiede and d'Ancona come to reproducing that is quoting (*Eyewitness* p. 65) Roberts' own paraphrase of it:

... the hand in which the text [of the Magdalen/Barcelona papyrus] is written is a carefully written book hand that may be regarded as a precursor of the style commonly known as Biblical Uncial (see a "Complementary Note" in Ramón Roca-Puig, *Un Papiro Griego del Evangelico de San Mateo* [2nd ed., Barcelona, 1962] 59-60).

Thiede and d'Ancona give emphasis to Roberts' choice of the term "precursor", but fail to acknowledge that Schubart's late second century dating for biblical uncials undermines the stated belief in *Eyewitness to Jesus* "... that the Magdalen Papyrus and its two sister fragments in Barcelona should be dated to the first century A.D., toward [*sic*] A.D. 70 or even earlier ..." (p. 106). In fact they resist coming to grips with that all-important issue throughout the book. Where discussion is called for, they choose to side-track the reader. This is most evident in their reaction to criticism leveled at Thiede for not citing (prior to *Eyewitness*) Guglielmo Cavallo's standard and magisterial work on biblical texts *Ricerche sulla Maiuscola Biblica* (Fiorenza, 1967): "... it was claimed that [Thiede] had overlooked [Cavallo], who states that this particular style [biblical uncial] was late, not early" (p. 106). Again but in greater detail they state

Applied to the Magdalen Papyrus, the category biblical uncial sounds good ... Thus those critics who drew attention to the biblical uncial and to ... Cavallo's standard textbook on this style appeared to have a point. But the redating had not ignored Cavallo or the biblical uncial; it had been carried out

in the spirit of objective analysis. The weakness of previous estimates was clear: certain key assumptions had persisted out of respect for tradition rather than because they were logically defensible. The question was: What really *is* the closest possible approximation--the demonstrable result of applied comparison or the most attractive and familiar category? (*Eyewitness* 114).

Such dissembling and pussyfooting continues into the two footnotes associated with that paragraph, notes # 10 and # 11 on p. 180. In note # 11 Graham Stanton is singled out for criticisms of Thiede in *Gospel Truth? New Light on Jesus and the Gospels* (1995) 14 where Stanton questions why Colin Roberts' late second century dating is doubted. The reply to this? "Stanton, in any case, misses the point of [Thiede's] original article [uncited], which was to explore fresh arguments rather than to criticize old ones." That approach, to employ a "smoke and mirrors" technique, imbues the entire text of *Eyewitness*. This means that in place of an open debate the reader finds circular arguments and flim-flam. We are nowhere near the fulfillment of the clear statement of the book's intent noted above: "It seeks to promote debate as much as to answer questions definitively" (*Eyewitness*, p. 5).

I have devoted much attention to the matter of what century the style of handwriting represents because it is central to the dating of the Magdalen fragments. If d'Ancona and Thiede cannot make a compelling argument for a pre-A.D. 70 date on that point, it is impossible to see how related aspects will persuade anyone with some knowledge of dating criteria. Choosing a "popular" book style as a forum for a subject embracing such esoteric, academic disciplines as papyrology, biblical text transmission, philology, and palaeography is odd to say the least.

Stranger still is the U.K. publication of a very "patchwork" collection of Thiede's articles, entitled *Rekindling the Word: In Search of Gospel Truth* (Fowler Wright, 1996). One of that group deals with the Magdalen scraps (translated into English from the German original). That volume has been reviewed (rather gingerly) by A.E. Harvey in the *Times Literary Supplement* (22 March 1996 p. 6). Thiede's *ZPE* exposition addressed exactly the audience that *P.*

Mag. Gk. 17 demands: those scholars whose critical opinion is absolutely essential to ensure that controversial claims can be substantiated. As noted above, a rejoinder to Thiede's piece has already appeared in that journal. It seems unlikely that similar publications will be any more sympathetic, and for the very same reasons. One need only recall that J.A.T. Robinson's *Redating the New Testament* (1976) was a valiant but decidedly vain attempt to retroject the entire NT canon to the period before the fall of Jerusalem.

Eyewitness to Jesus does nothing to advance Thiede's claim, and much to subvert it. The flawed methodology, the oblique and often opaque argumentation, lack of a proper *apparatus criticus*, and the proclivity to engage in parenthetical or otherwise irrelevant discussions are clearly indicative to me that subterfuge has been substituted for substance. It is very obvious that our attention is purposely being diverted. Let us look at some specific examples. A discussion of the known facts concerning the provenance of the Magdalen papyrus is relegated to chapter four, and even then the authors fail to note that Huleatt may have acquired more than the three fragments he sent to Oxford. The photographs of those fragments appear in two half-plates, *verso* in the top half, *recto* in the bottom half. None is numbered and all are difficult to see clearly without the aid of a magnifying glass. By way of contrast the photo of the Qumran Greek manuscript fragment 7Q5 is so large and clear (it occupies an entire plate) that the horizontal and vertical alignment of papyrus strips can be seen!

Eyewitness contains no transcriptions of the texts on the various Magdalen fragments. This is a deliberate omission since Thiede's *ZPE* article provided clear transcriptions of the Greek. Readers are offered instead translations of the relevant portions of *Matthew 26*. Without any hesitation, the authors defend this unconscionable procedure by saying:

It may facilitate our stroll through this colorful but confusing thicket if we read the translated English text of these fragments from St. Matthew 26 first, quoting the *New Jerusalem Bible* ... In printing this translation, we have not tried to copy the line divisions of the Greek fragments or the

fragmentary character of words at the beginnings or ends of some lines; and, needless to say, since Greek syntax differs markedly from English syntax, the order of words within the sentences is different as well ... But this provides a workable idea of what is in these fragments ... (*Eyewitness* pp. 56; 57).

"Needless to say" it is then impossible to follow arguments (p. 58) regarding alleged abbreviations of *nomina sacra* (such as IHCOUC = $\bar{\text{I}}\bar{\text{C}}$, KYPIOC = $\bar{\text{K}}\bar{\text{C}}$) on several fragments, or the numerical abbreviation IB for *dôdeka* on one fragment. It is clear that the authors find it very advantageous to reverse the usual procedure by working back *from* a standard translation *to* their theoretical proposal of what the Magdalen scraps actually contain. This may fool the reader or the reviewer with no knowledge of how textual analysis is done, but it is a blatant violation of the rules of scholarly debate. This isn't a "stroll" through a "thicket" but an opportunity to enter a hall of mirrors at the end of which is a smoke-filled *cul-de-sac*. A sign should warn: *CAVEAT LECTOR*.

The very fact that the fragments are from a codex format of Matthew, and the text may contain abbreviations of *nomina sacra*, militates against a first-century date for *P. Mag. Gk. 17* as well as the Barcelona papyrus scraps. So does their provenance in the interior of Egypt. It is one thing to argue that the fragment of a Greek papyrus scroll from Qumran (7Q5) may be a portion of the *Gospel of Mark* dating before A.D. 70, and another to argue that fragments of a Greek codex of the *Gospel of Matthew* from Upper Egypt are of that same date. It should be noted that Thiede has been a strong supporter of Fr. O'Callaghan's belief that 7Q5 is a portion of *Mk. 6:52-53*: this is set out in an article for *Biblica* 65 (1984) 538-59 and in his *Die älteste Evangelien-Handschrift?: Das Markus-Fragment von Qumran und die Anfänge der schriftlichen Überlieferung des Neuen Testaments* (1986).

However much he champions that cause (which continues today to attract some support) or refers to the papyrus discoveries at Herculaneum, Thiede's (and d'Ancona's) reliance on them must be seen as a counsel of despair. Dating arguments based on analyses of palaeographical similarities are notoriously unreliable. Just the fact that every legible *sigma* in the Magdalen papyrus (I can count six in

Thiede's *ZPE* photographs) is lunate (C) in shape is an additional reason to date the fragments in the second century. In photos of the fragments (I have not seen the scraps myself) I count only about fifty clearly legible letters, perhaps a third of the total on all six sides. From such a small sample one needs more faith than I have to argue the question of date. The papyri from Herculaneum were recovered in unusual circumstances: a very controlled archaeological context sealed by volcanic deposits. I would not guarantee, as Thiede and d'Ancona would have it, that a similar situation obtained in the Qumran caves. Fragment 7Q5 may very well be from the *Gospel of Mark*, but to insist that the cave in which it and other papyrus scraps were found had been somehow hermetically sealed from intrusion after A.D. 70 is not wise.

The tiny quantity of the Magdalen Papyrus fragments and the uncertain circumstances of their provenance are (unfortunately) the prime ingredients for maximum speculation. Perhaps a rule of historical research is at work here: hypotheses expand in number and complexity in inverse proportion to the amount of credible documentation available. That is not the only "maxim" we need to note: supporting one uncertainty by invoking a second of equal or greater uncertainty is another. Thiede and D'Ancona argue from alleged fragments of the New Testament at Qumran that the Magdalen and Barcelona fragments of the *Gospel of Matthew* are also pre-A.D. 70. Such circular reasoning goes nowhere unless the reader does not recognize it as such. Without doubt *Eyewitness to Jesus* will persuade and even convince some who mistake its shell-game manner of presentation for a scientific evaluation of evidence. Clearly it retails a position established and maintained by presumption, a position which ignores the probative method of open debate.

It may not be amiss to say a few words about this procedure because questionable New Testament research has attracted more than its usual share of public attention in the past decade. The high-profile "Jesus Seminar", founded by Robert Funk in 1985, has been central to the commercial aspect of this. In some ways it's become a theological "lightning-rod", drawing bolts of criticism from both fundamentalists and liberals. The Jesus Seminar's very *raison d'être*,

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its methodology and its publications (individually or collectively) have just received a thorough (and disapproving) appraisal by a mainstream biblical scholar, Luke Timothy Johnson, in *The Real Jesus: The Misguided Quest for the Historical Jesus and the Truth of the Traditional Gospels* (Harper San Francisco, 1996). Serious New Testament scholarship is well-served by it.

Johnson does not confine himself to just a critique of the Jesus Seminar or its academic staff. He ranges far and wide over the past decade, embracing a broad spectrum of publications which focus on some aspect of earliest Christianity: Jesus, Paul, the formation of the Gospels, Christian communities and their alleged relationship to the Essenes in general or to the Khirbet Qumran settlement in particular, the development of the New Testament canon, Gnosticism and other varieties of early Christianity, and the extra-canonical corpus (apocryphal writings of all sorts). He includes a brief aside (*The Real Jesus* p. 78), and not at all in a complimentary manner, on a *Time* magazine story entitled "A Step Closer to Jesus?" (23 January 1995) about the Magdalen fragments.

For the most part Johnson's concern is to analyze the main features of this "Jesus phenomenon" and to expose the sometimes ludicrous manifestations of shabby scholarship aided and abetted by editors and publishers eager to cash in on controversy which those very authors and an uncritical media generate and sustain. *The Five Gospels: The Search for the Authentic Words of Jesus* (1993) was the Seminar's first collective report and therefore gets plenty of attention from Johnson (*The Real Jesus* pp. 20-27). Beyond that he examines recently-published works promoted as the vanguard of "historical Jesus research" in an attempt to isolate or identify the salient features of this genre of biblical lore. Much of Johnson's energy is then devoted to defending mainstream religious scholarship, and traditional Christianity, from what he understands as the central purpose of the Seminar and its devoted adherents:

... the agenda of the Seminar is not disinterested scholarship, but a social mission against the way in which the church controls the Bible, and the way in which the church is dominated by a form of evangelical and eschatological theology ..." (*The Real Jesus*, p. 6).

For those who have no knowledge of the history of this genre of literature, Johnson's assessment of it in *The Real Jesus* may seem more comprehensive than his specifically limited intention. As far back as the 1960s several biblical scholars with solid and sometimes international reputations began to produce a series of publications which eventually relegated them to the far edges of respectability within their field of research. There was nothing like the Seminar to draw individuals together. Jesus was boldly presented as a deluded schemer (*The Passover Plot*, 1966) or as a card-carrying revolutionary (*Jesus and the Zealots*, 1968) or as a charismatic magician (*The Secret Gospel*, 1973) or as the earthly manifestation of a pre-Islamic Arabian fertility god (*Conspiracy in Jerusalem: The Hidden Origins of Jesus*, 1988). Christianity itself was "reinterpreted" in several radical and revisionist "studies", ranging from a characterization of it as a psychedelic cult (*The Sacred Mushroom and the Cross*, 1970) to "unlocking" its hidden origins as the religious step-child of a Qumran sectarian movement (*Jesus and the Riddle of the Dead Sea Scrolls*, 1992).

None of this has gone unnoticed. In his magisterial and well-received study *The Death of the Messiah: From Gethsamene to the Grave. A Commentary on the Passion Narratives in the Four Gospels*. (2 vols., Doubleday, (1994), Raymond Brown devoted a whole section (pp. 1092-6) to this topic, which he accurately entitled "imaginative Rewriting that Nullifies the Crucifixion." His own summary is worth quoting in full:

The survey above shows that there is not likely to be much new under the sun in such exercises of the imagination. These theories demonstrate that in relation to the passion of Jesus, despite the popular maxim, fiction is stranger than fact -- and often, intentionally or not, more profitable" (p. 1096).

All of these books share one feature in common: each wants to be taken seriously as biblical scholarship even if its theme or focus or central argument is quite controversial and cannot be supported by a close scrutiny of what each adduces as "evidence." Every book mentioned above was written by a scholar or academic. Somewhat like Matthew's Jesus, these authors want to straddle *two* donkeys, hoping that such an awkward position will go unnoticed amidst the

approval of an enthusiastic reception. *Eyewitness to Jesus* is no exception to that wishful thinking, although its own scope is narrow in comparison. Thiede and d'Ancona want to assign an exceptionally early date to the Magdalen Matthew fragments. To do so they base their argument on palaeographical analyses that are at best extremely ambiguous and at worst little more than an example of pseudo-scholarly *trompe l'oeil*.

Without doubt *Eyewitness to Jesus* will earn its authors much money. Jesus is big business in the publishing industry and sells well as a media-hype package. Easter Week 1996 in the U.S.A. saw three popular magazines feature cover stories with titles such as "In Search of Jesus" (*U.S. News & World Report*), "Rethinking the Resurrection" (*Newsweek*) and "The Search for Jesus" (*Time*). Only the last-named gave any attention to the Magdalen papyrus (p. 60) but all three confirmed Prof. Johnson's belief that "Commerce in the Christ has rarely been better" (*The Real Jesus* p. 1).

How to understand "better" is certainly not difficult. It is now possible for biblical scholars of very modest talents to earn huge supplemental incomes. One ivy-league professor received an advance of nearly half a million U.S. dollars for a book MS which several colleagues in the field found so lacking in substance and focus that they provided pages of critical comments and suggested changes. These were ignored by author and publisher, and the book appeared on schedule. The author's colleagues who had recommended substantial modifications were thanked in the acknowledgements as if they had provided the book's *nihil obstat* and *imprimatur*. The volume has sold well, in part because the publisher arranged for the author to be profiled in popular journals and to be available for interviews on the "talk-show" circuit.

The author's sumptuous advance was publisher's money well spent and (of course) a sequel volume is now in progress. Since the trio of Easter-week cover stories (noted above) adorned all U.S.A. newsstands, two very different interpretations of "what Jesus was" have moved onto the "best-seller" lists in the U.S.A.: Reynolds Price's non-fiction *Three Gospels* (1996) and Paul Park's novel *The Gospel of Corax* (1996). The former is Price's somewhat odd re-translation of the Gospels of Mark and John, followed by a narrative

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recreation of a "Life of Jesus" which Reynolds styles (incorrectly) "An Apocryphal Gospel". The latter is a bizarre but imaginative account of Jesus' "hidden years" as a novice Buddhist monk, somewhat in the tradition of such fanciful works as Robert Graves' *King Jesus* (1956) or Nikos Kazantzakis' *The Last Temptation of Christ* (1958).

Neither Price nor Park is a biblical scholar or an ancient historian and neither makes any claims to such a background. They cannot and should not be judged by the same scale of values used for those who are trained to be, and make claims to be, biblical scholars. But Price, Park and others benefit from the antics of those who are members of what Prof. Johnson terms "the academy" (the scholarly establishment) and yet display in their attitudes and publications little concern for or attention to the standard rules of academic research. As this millennium's end approaches, the need to satisfy a perceived public interest in the arcane or revelatory or apocalyptic or anti-establishment aspects of early Christianity will most certainly lure many more scholars into the marginal but very lucrative market for "Jesus books". If obscure academics can make cover stories out of Christ, why shouldn't *any* writer of even mediocre talents do the same?

Some years ago I had dinner with Morton Smith during a dull conference at Brown University. In the course of a long and very wide-ranging discussion I asked him why it had taken him fifteen years (1958-1973) to publish his controversial *The Secret Gospel of Mark*. He looked at me very carefully and replied: "I didn't have any competition. There didn't seem to be any need to rush it into print." That is certainly not the case with *Eyewitness to Jesus*. Thiede and d'Ancona must have realized that the longer they waited between the initial article in the London *Times* and the publication of the book reviewed here, the less chance they had of capitalizing on the sensational aspects of their claims.

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Gillian Keys, *The Wages of Sin: a Reappraisal of the 'Succession Narrative'* JSOTS 221; Sheffield Academic Press, 1996; ISBN 185075621X; 244pp.hb.

The target of Keys' work appears from the inverted commas in the title. The Succession Narrative (SN) has been one of the sacred cows of twentieth century Old Testament criticism. The thesis, propounded famously by L. Rost (though he had antecedents), is, in brief, that the section of 2 Samuel that deals with David's reign is a self-contained composition, dating from or close to that reign itself, and having as its theme the identification of a successor for King David. Rost enjoyed the support of such heavyweights as G. von Rad, who regarded the author of SN as an eyewitness of events at David's court. The limits of the work, as usually defined, are 2 Samuel 9-20; I Kings 1-2, thus omitting 2 Samuel 21-24, a section that has been dubbed the Samuel Appendix, with all the hints of redundancy implied by that term. Samuel scholarship, therefore, has sought to describe how an originally independent block such as SN came to be part of the books of Samuel as we know them, and indeed of the larger so-called Deuteronomistic History (Deuteronomy-Kings).

Keys shows first how there has been less consensus on the theory of SN than may appear from textbooks. It has, in fact, been challenged on the grounds of extent, unity, theme, genre and purpose. These topics in its study interrelate: a decision about its theme, for example, depends on whether I Kings 1-2 is an integral part of this narrative. In her own treatment of the theme she rightly argues that Rost underestimated how much the narrative focused on David himself, because of his preoccupation with the idea of succession.

As for the extent of SN, Keys rejects the view that I Kings 1-2 belongs intrinsically with the narrative in Samuel, because of differences in style, language, content and theology. For example, the chapters in Kings are more theologically articulated, and more cultic in their character. The beginning of the narrative has caused its own problems to advocates of the theory. 2 Samuel 9:1 is too abrupt to be a true beginning, and therefore a beginning has to be

supplied either from outside the present narrative, or from an earlier part of it. The story of Michael is a leading contender (2 Samuel 6:16-23). Keys rightly argues, however, that to co-opt this exclusively for the theme of succession misses its important connection with the Ark narrative in whose context it is found. In her argument that the claim of the traditional theory of SN to have properly defined an originally self-contained piece of literature is unsatisfactory, she has made an important contribution to Samuel studies.

Keys goes on to develop a hypothesis about the authorship of the Books of Samuel that supposes a development entirely within the parameters of those books. She thinks the story of David as king originally comprised 2 Samuel 10-20; this was incorporated into the rest of 2 Samuel, including the so-called 'Appendix', which, as she rightly argues, echoes the theme of David and Saul, so central to the storyline of Samuel. The author of 2 Samuel then attached his book to 1 Samuel. And finally 1 and 2 Samuel were built into the Deuteronomistic History (DtH) 'as a block'.

The broad purpose in preserving this record, and in incorporating it into DtH, was '....to provide a record of the reign of David'. Further, now that she has delineated the correct limits of the story of King David, she redefines its theme, which is twofold, namely (a) the wages of sin, and (b) David the man. David's sins of adultery with Bathsheba and murder of Uriah result in the tragedies that then overtake his family. The focus in the story is on David as an individual, and not on the politics of his reign.

The specifics of this thesis cannot entirely escape the reviewer's quibble. The thesis that 2 Samuel 10 is the true beginning of the story of David's reign, yet 'not completely independent of the surrounding material' (p.80), raises a question that is not fully answered. 2 Samuel 10:1 arguably suffers as many handicaps as a 'beginning' as other new departures within the Samuel narrative do. Her thesis about the growth of the Books of Samuel is certainly possible, but I think the story of the composition of Samuel may be more complicated, and that Keys has perhaps not challenged the idea of SN radically enough in the end. (It is a little surprising to find that the term survives in her

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treatment). She has also said little about how Samuel has come to be incorporated into the larger history. (Her argument that very similar but slightly different phraseology in 2 Samuel 15:1 and 1 Kings 1:5 bespeaks different authors, for example, could be turned the other way, and she does not tackle the difficult question of how the correspondence between the passages occurred; pp.64-65)

I think too that there is more to the narrative than is encapsulated in the two themes proposed. The shift away from 'succession' is welcome, and the focus on the life of David is right. But there is an unhelpful division between David the man and the political fabric that is unfolded here. Indeed it is the relationship between the life of the individual and that of the body politic that is really intriguing in the story of King David. It is not incidental that David is king. It is this that produces the family tragedy, as is well caught in the hard man Joab's impatience with David's soft underbelly, revealed in his love for his son (2 Samuel 19:5).

Nevertheless, Keys' thesis is welcome, and timely. Its strength is in its careful unpicking of the elements that hold together a powerful theory, and the demonstration in the process of how such theories are perpetuated. The timeliness of it lies perhaps in the fact that there is a mood for the re-evaluation of received ideas. SN itself has become more controversial than ever before, especially in its implicit or explicit claim to historical reliability. Studies of DtH too have reached a stage of great diversity, with widely differing estimates of its status as history. In parallel with these developments in historical studies, there has been a movement towards the study of the individual books that make up DtH, as entities in themselves. One great advantage of Keys' book is that it does not adopt the all too common device of separating literary and historical study. She has considered the theme of 2 Samuel in tandem with a study of the book's historical context and development. And her thesis will have to be reckoned with in future studies of Samuel and DtH.

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